# Entrepreneurship and State Policy

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**Abstract**

Entrepreneurship plays an important role in labor markets, productivity growth, and occupational choices. While a large and growing literature studies patterns in entrepreneurial activity in the U.S., there exists little well-identified research into the policy determinants of entrepreneurial outcomes and the differing effects of policies on firms of different ages. Using the recently developed Quarterly Workforce Indicators dataset, we consider three state-level policies—corporate income taxes, minimum wages, and personal income taxes—and study their effects on new firm activity by comparing contiguous counties that lie across state borders. We estimate the effect of changes in these policies on employment and job flows at new firms. We find significant negative effects of corporate tax increases on the level of entrepreneurial activity, and we find that new firms account for a disproportionate share of the response of aggregate employment growth to such tax changes. The effects of minimum wages are of moderate size but largely dissipate after accounting for cross border spillovers. Surprisingly, we find no statistically significant impact of personal tax rates.

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# 1 Introduction

Entrepreneurship is a driver of crucial economic outcomes. New firms make significant contributions to both gross and net job creation (Haltiwanger *et al.* 2013), play a major role in business cycles (Adelino *et al.* 2014; Pugsley & Sahin 2015), and account for an outsized share of the innovation and aggregate productivity growth that raises living standards (Bartelsman & Doms 2000; Foster *et al.* 2001; 2006; 2008). Additionally, entrepreneurship is seen by many as an important element of the occupational choice set (Hurst & Pugsley 2011). Given its importance, it is not surprising that there exist large strands of literature on the economic impacts of entrepreneurship, national trends in entrepreneurial activities, and correlations between various policies and entrepreneurial activities. However, research on the policy determinants of entrepreneurship has been hampered due to limited data on entrepreneurial activity and a lack of credible exogenous variation. As a result, our understanding of how policies affect new firms and entrepreneurial job creation remains surprisingly limited.

This study overcomes these hurdles by using the newly released Quarterly Workforce Indicators (QWI) firm age dataset, which provides detailed county-level information on job creation, job destruction and other key labor market variables for firms in narrowly defined age categories, including firms with age less than two years. Using this new data resource, we isolate plausibly exogenous variation in state-level corporate taxes, minimum wages and personal tax rates over time and across state borders. Specifically, we examine how entrepreneurial activity changed in counties that experienced a change in their state corporate tax rates, minimum wage and personal income tax rates relative to bordering counties whose state did not change corporate tax rates. We use a variety of specifications that exploit different levels of variation and explicitly test for the presence of spillovers across borders. As in much of the recent literature, we adopt an age-based definition of entrepreneurship; in particular, in the present study we define “entrepreneurs" as firms with age less than two years.

To the best of our knowledge this is the first study to attempt and recover credibly identified causal estimates of the impact of state policies on entrepreneurial activity. Using our border county identification strategy, we examine the impact of these policies on both the levels and the trends in entrepreneurial activity. We find that increases in corporate tax rates have a statistically and economically significant negative effect on employment, with the effect being larger for new firms than for all firms. Minimum wages moderately reduce employment growth at new firms generally but have only small effects on employment levels. Personal tax rates have no significant effect on employment. We explicitly examine the extent to which our results reflect the shifting of activity across state borders. Identifying the spillover ef-

fects of these policies allows us to test the internal validity of our estimates and identify entrepreneurs' ability to relocate their economic activity. Spillovers across borders are not observed in response to changes in corporate tax rate changes but are observed in response to changes in the minimum wage. Results are robust to a variety of specifications including models that omit states in which a majority of activity occurs along borders. We compare our results to a straightforward state-level panel model whose estimates, when statistically significant, overstate the negative effects of corporate taxes on employment, highlighting the value of our border county approach.

One reason that entrepreneurship has received significant attention in recent years is that rates of entrepreneurship in the U.S. have been declining for several decades (Decker *et al.* 2014). Declining rates of entrepreneurship may be a concern for three reasons. First, entrepreneurship is seen by many as an important occupational choice consistent with lifestyle preferences (Hurst & Pugsley 2011). While a decline in this type of "lifestyle" entrepreneurship may be benign for aggregate job and productivity growth, it may represent intensifying scarcity of opportunities to pursue a preferred occupation. Second, a small number of entrepreneurs typically grow rapidly, ensuring that high startup failure rates are offset on net by significant job and productivity growth (Decker *et al.* 2014). Third, declining rates of firm entry have been associated with a declining pace of gross job flows and worker reallocation; these measures of labor market fluidity are an important source of wage and productivity growth (Hyatt & Spletzer 2016; Syverson 2011). Fully understanding the consequences of declining entrepreneurship of any type requires evidence frictions that reduce entrepreneurial activity. Our estimates of the policy determinants of entrepreneurial activity shed light on these broad trends and suggest further avenues for research and policy. Notably, while we find that corporate taxes are associated with lower entrepreneurial employment, this is not likely to be a key driver of aggregate declines in entrepreneurial activity since the general trend in corporate tax rates has been negative. In this respect, our results deepen the puzzle of declining aggregate entrepreneurship, though our approach may be a useful pattern for further investigation of the patterns using state variation.

This paper fits with a growing literature that attempts to examine the role subnational policies play in determining entrepreneurial and reallocative outcomes. For example, Autor *et al.* (2007) find that wrongful discharge protections reduce entry of new establishments. A small literature has also emerged studying the impact of environmental regulations on economic dynamics. Walker (2011) finds declines in job creation and List *et al.* (2003) find declines in new plant births in response to the Clean Air Act's nonattainment standards. Curtis (2014) finds declines in worker turnover in response to the NOx Budget Trading Program. Re-

sults from existing literature on other policies are mixed, and these studies often do not derive causal estimates of the effects of the policies on new firm formation and growth. Using panel regressions, Garrett & Wall (2005) find a negative relationship between entrepreneurship and corporate tax rates, minimum wages, and stringency of bankruptcy laws, with no relationship with personal tax rates. Primo & Green (2011) find similar results for bankruptcy law but no relationship for taxes, and Goldschlag & Tabarrok (2014) find no relationship between federal regulation counts and various measures of economic dynamism. Ours is the first study to employ plausibly exogenous policy variation to study entrepreneurial outcomes.

The policies studied in this paper have received considerable attention in other contexts. Corporate and personal taxes have increasingly been studied in recent years as important drivers of firm location, employment flows and other economic outcomes (Ljungqvist & Smolyansky 2014; Giroud & Rauh 2015; Suárez Serrato & Zidar 2014; Akcigit *et al.* 2015; Fajgelbaum *et al.* 2016). An extensive literature has examined the consequences of minimum wages for employment levels and worker flows (Card & Krueger 1994; Dube *et al.* 2010; Neumark *et al.* 2014; Dube *et al.* 2016; Meer & West 2016). Our contribution is to focus specifically on the implications of these policies for entrepreneurial activity as well as allow for their joint determination. There are additional policies which we may analyze in future versions of this paper.[1]

The remainder of the paper is organized as follows. Section 2 provides background information and details of the policies studied. Section 3 describes the main data sources of the paper. Section 4 provides the econometric models used in the paper and the results from those models. Section 5 discusses the results. Section 6 concludes.

# 2 Policy Background

The three polices this paper explores are corporate tax rates, the minimum wage and personal income tax rates. Corporate taxation policy is far from straightforward in the United States. Most states use income taxes that are similar to the corporate taxes imposed at the federal level. Some states, however, impose their primary corporate taxes on gross receipts, asset base, or other business outcomes, while a few states have no business tax of any kind. For our purposes, we focus only on income taxes; as shown on Figure 1, there is a fair amount of change in these rates over time. Additional corporate tax variation comes from legal form of organization concerns. Historically, most firms in the United States were classified as C-

[1]See Wilson (2009), Chirinko & Wilson (2014) and Rohlin *et al.* (2014) for research on the impact of other policies (such as job creation creation tax credits, R&D credits and reciprocal agreements between states) on overall business and employment levels.

corporations and were subject to federal and state corporate tax rates. As discussed in Cooper *et al.* (2015), "Pass-through" entities, which are typically subject to different tax rates, have become increasingly popular in the past thirty years. Our county-level data do not allow us to distinguish between C-corporations and pass-through entities.

There is an important point about legal form of organization to be made here. In most states, firms organized as LLCs, S-corporations, sole proprietorships or partnerships will not be directly affected by changes to the corporate tax rate. The earnings of these firms are subject to personal income tax rates. Entrepreneurs may select different organizational forms based on existing corporate and personal tax rates (Giroud & Rauh 2015). With our data we are not able to isolate the impact of corporate tax changes on C-corporations. Nonetheless, the likely heterogenuous effect of tax rates on firms in no way diminishes the importance of a key question: how do tax rate changes affect *overall* employment at firms of various ages? If few firms are affected or if entrepreneurs choose a different legal form of organization in response to changes in relative tax rates, then we will find muted, or perhaps zero, effect of taxes on overall employment. This finding would certainly be relevant. Given that there are many firms unlikely to be directly impacted by changes to the corporate tax rate, the fact that we still find strong effects of the corporate tax rate implies that there are many firms that are affected or that affected firms face large effects. We argue that this overall effect remains a vitally important policy and economic parameter, and it strongly suggests that the impact on C-corporations is larger than the estimated overall effect.[2]

States exercise broad discretion in setting minimum legal wages. Employers in states with no minimum wage (or a minimum wage set lower than the federally legislated minimum wage) are subject to the federal minimum wage; this situation is rare enough that there still exists significant variation across states in minimum wage levels and changes over time (see Figure 2). Minimum wages have been the subject of intense study and policy debate; for a summary see Meer & West (2016). Minimum wages have already been found to affect business dynamism in the form of worker flows (Dube *et al.* 2016). In principle, they may affect entrepreneurship specifically if new firms are more likely to attract low-productivity workers, or due to the "integer problem" of increasing employment from a low initial level.

Most states impose personal income taxes that apply in addition to federal income taxes (though, in some cases, states allow federal tax payments to be deducted from state taxable income). These tax rates are directly relevant for entrepreneurship because many en-

[2]There is limited available public data on business' legal form of organization (LFO). The County Buisness Patterns contains state level employment by LFO starting in 2010. Early attempts to exploit state level variation in LFO to identify the differential impact of personal and corporate taxes has not significantly altered our main findings.

trepreneurial ventures are organized as pass-through entities. In most states, business income earned in pass-through entities is subject to personal income taxes. Additionally, states' treatment of capital gains may deter or incentivize venture capital activity, a key source of funding for startups. Additionally, entrepreneurs are frequently compensated through stock options whose payoff depends on firm performance. These future income from these stock options is taxed at capital gains tax rate which varies considerably across states. The treatment of capital gains taxation has long been thought to be a determinant of startup activity (Poterba 1989a;b) but there remains limited empirical work on this subject. [3] As with corporate taxes and even the minimum wage, entrepreneurial activity may be affected by personal taxes through other general equilibrium mechanisms. Figure 3 shows historical movements in state personal tax rates.

While including multiple state-level policies in our analysis, we would expect different types of policies to have different effects on entrepreneurship. For example, corporate or personal income taxes are (roughly) taxes on profits. In a simple model, the primary effect of such a tax is to reduce entry by reducing lifetime expected profits (which must be compared to entry costs). After entry, profit taxes might have little or no effect on factor demand decisions such as employment. Hence, a profit tax might be expected to primarily affect the extensive margin of entrepreneurship. In richer models, though, profit taxes may affect more margins. Unfortunately, we lack the data detail necessary to distinguish between extensive and intensive margins of entrepreneurship.

A minimum wage acts very differently from a profit tax. In a simple model, this may be thought of roughly as a tax on low-wage employment. Such a tax can reduce lifetime expected profits and, therefore, affect the extensive margin of entrepreneurship as does an income tax. In addition, though, it directly affects the labor demand decision and the optimal ratio of labor to capital (or of low-skilled labor to high-skilled labor). This persistent effect on the intensive margin of production means we might expect impacts over the lifecycle that differ from the impact of income taxation. A minimum wage is a recurring wedge in employment decisions even among incumbent firms. One might still expect young firms to be affected more by minimum wages due to extensive margin effects, but it is also possible that young firms labor demand is less elastic than that of older firms due to greater ability of older firms to substitute toward capital. Ultimately these are empirical questions, but these theoretical considerations imply that income taxes and minimum wages may have very different effects

[3] It should also be noted that states differ in the apportionment formulas they use to divide corporate income taxes. Many states equally weigh companies' payroll, sales and property to determine economic activity but in recent years, states have shifted to place higher weight on sales rather than employment. Regressions that interact the corporate tax rate with the payroll apportionment weight do not meaningfully change the main results of the paper.

on our measured entrepreneurial outcomes.

One benefit of examining these three policies together in the same regression framework is that states' decisions to change one policy may occur simultaneously with changes they make to other other policies. If changes in policies occur simultaneously then regressions that focus only on one policy are likely to be biased. The direction of this bias is not always clear. A state may, for example, increase corporate tax rates in order to compensate for reducing personal tax rates. On the other hand, negative shocks to a state's budget may require it to raise all taxes simultaneously. Economists have similarly argued over the timing of states' minimum wage increases (Meer & West 2016). Minimum wage increases may occur when states' economies are performing relatively well. If other policies are passed during economic booms then estimates of the minimum wage that fail to observe these other policies may be biased. Our flexible approach allows us to control for each of these policies while also examining them individually. Furthermore, the border discontinuity method exploits variation in policy changes that are unlikely to be correlated with changes in entrepreneurial or economic activity.

# 3 Data

The primary dataset used in the empirical analysis is the publicly available Quarterly Workforce Indicators (QWI). The QWI is derived from Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program which gathers administrative data from states' unemployment insurance programs, social security records, the Longitudinal Business Database and the Decennial Census among others.[4] The LEHD combines these data at the worker and firm level and uses it to create a public use version at the county level. Within a county, a number of variables are reported for different worker and firm categories. Specifically, private sector employment, job creation and job destruction are reported separately for five firm-age categories: new firms (0-1 year-olds), 2-3 year-olds, 4-5 year-olds, 6-10 year-olds, and firms 11 years old or older. For the present study, we use the terms "startup" and "entrepreneur" interchangeablly to refer to new firms (having age less than two); these startup firms are the primary focus of our analysis. It is important to note that firms enter the QWI scope when they hire their first W-2 employee; hence, the entrepreneurs we are studying are employer businesses.

There are two unique features of the QWI that make possible the type of analysis performed in this paper. First, no other public dataset contains county-level data on startup

[4]See Abowd *et al.* (2006) for details on the QWI's construction.

firms. Commonly used data sources on startups, such as the Business Employment Dynamics and the Business Dynamic Statistics contain information only at the state or MSA level. Second, the QWI provides information on both net and gross employment changes. Job creation (the number of additional jobs at expanding establishments) and job destruction (the number of lost jobs at contracting establishments) are reported to better understand the types of reallocation occurring in the economy.

Figure 4 displays the specific counties that are used in the primary border discontinuity analysis. Ohio, Texas and Michigan are dropped from the dataset because they tax gross sales rather than profits. Counties that are in orange belong to states where more that 50 percent of their employment is located in counties that border another state. These states are excluded in some of the robustness checks.

The primary results explore four different outcome variables for startup firms in these counties: logged employment, employment growth, job creation and job destruction. Separately examining logged employment from employment growth allows us to understand how these policies impacted both levels and trends in entrepreneurial and economic activity. Panel models may be slow to detect changes in levels if these changes evolve slowly overtime. However, changes in trends will more quickly show up in the data. Ideally, an event-study model could be used which captures the dynamics of the variable of interest over time. As seen in Figures 1-3, the frequency of within-state policy changes make event-studies particularly difficult to estimate. Many states phase these policies in over multiple years. Furthermore, they often adjust these policies multiple times over the sample period. This makes an event-study analysis unreliable as there is no singular clean shock whose effects can be tracked out over time.

Job creation and job destruction are also evaluated to understand the impact of these policies on reallocative activity. To construct the creation, destruction and employment change variables, levels of job creation, job destruction and employment change are scaled by the county's 2006 employment level. This is similar in spirit to Adelino *et al.* (2014) who scale these outcomes by a county's employment in 2000. Scaling in this way allows for job creation in a county to be comparable across time and across firm groups. The year 2006 is chosen because it is the first year in which all participating states provide data.[5] The study period consists of the years 2000-2013, and counties with fewer than 3,000 workers in 2006 are dropped from the sample. [6]

[5]90% of states provide data for the entirety of the 2000-2013 study period. Note that there is an employment change identity whereby for each unit of observation $\Delta Emp_t = Emp_t - Emp_{t-1} = Creation_t - Destruction_t$.

[6]Results are not sensitive to this data restriction. Dropping small counties is useful for two reasons. First, the QWI suppresses data if there are few workers or firms in a particular county. In principal, suppression itself

County-level data on startups and firm reallocation are merged with a number of other datasets to obtain control variables and information on the policies of interest. The Census Bureau's Population Estimates Program provides annual county-level population data. State corporate tax rates are drawn from the Tax Foundation and are supplemented with information from the Book of the States and state tax forms. Corporate tax schedules vary from state to state. Following Ljungqvist & Smolyansky (2014), we focus on changes in states' top statutory marginal tax rate. With few exceptions corporate tax rates are levied on firms' profits.[7] While the specifics vary from state to state, corporate taxes are levied based on economic activity in a state rather than the location of the company's headquarters. The minimum wage data are obtained from Meer & West (2016) and are updated through 2013 using the U.S. Department of Labor's State Minimum Wage Reports. States' nominal minimum wages during the sample period are adjusted into constant 2011 dollars using the national CPI deflator. Personal income tax data are obtained from NBER's TAXSIM model. We use the reported maximum state income tax rate as a proxy for the personal tax rate that potential entrepreneurs would face.[8] Data for each of these policies are at the state-year level.

## 3.1 Characteristics of Young Firms

Given our focus on young firms, it is useful to provide some basic details about their characteristics. Consistent with existing literature (e.g., (Haltiwanger *et al.* 2013)), data from the Census Bureau's Business Dynamics Statistics (BDS) show that startups (firms with age 0-1) are overwhelmingly small.[9] In 2000, the year in which our analysis begins, firms with fewer than 10 employees accounted for about 90 percent of startups and about 40 percent of startup employment; these numbers changed very little over the period 2000-2014. Large firms (those with 500 or more employees) accounted for about 0.03 percent of startups and just under 7 percent of startup employment. The average size of startups throughout the period studied is

could be a function of the policies of interest. For example, if the number of new firms in a county drops to one or zero as a result of increased corporate tax rates then the county's data will be suppressed in that year. A second issue is that small counties have far more variation in the outcome variables. For small counties, even relatively minor creation or destruction events will result in large swings due to the small denominator. For the border discontinuity results we require that both counties in the border pair have at least 3,000 workers in 2006, the first year for which data is available in all states. Table A3 reports results based on the reduced threshold of 1,000 workers in a county.

[7] The exceptions are Ohio, Texas and Michigan which we exclude from our analysis.

[8] This measure is calculated by the Taxsim program and frequently used by researchers. See `http://users.nber.org/~taxsim/state-rates/` for further details. While the data used in this paper is only for earners in the top income brackets, this paper plans to explore lower tax brackets as well, particularly since many entrepreneurs operate at a loss in the early years of their buisness.

[9] QWI does not allow for studying firm age by firm size

about 7 employees, with the skewness of the distribution implying that the median is lower yet.

Startups in the QWI are more likely than older firms to employ young workers.[10] In 2000, workers with age less than 25 accounted for about 21 percent of startup employment compared with 17 percent of overall employment. Startups also disproportionately employed workers aged 25-34 in 2000, which comprised 26 percent of startup employment versus 24 percent of overall employment. By contrast, workers aged 35 or older account for 52 percent of startup employment compared with 59 percent of employment generally. These patterns hold qualitatively throughout 2000-2014.

Startups also differ modestly from other firms in terms of the education of their workforce. Setting aside those workers for which education data are not available (those of age less than 25), startups employed more workers lacking a high school diploma than did firms generally, with 13 and 15 percent of employment, respectively, in 2000. Similar gaps are apparent throughout 2000-2014. Shares of workers possessing only a high school diploma (or equivalent) are roughly similar between startups and firms generally. The share of employees with a bachelor's degree or higher was about 27 percent for both startups and firms generally in 2000, but in later years startups fell behind other firms in this measure. Broadly speaking, startups tend to employ somewhat less educated workers than do other firms.

Startup activity varies widely by sector, ranging from less than 2 percent of employment in utilities (NAICS 22) to over 9 percent of employment in accomodation and food services (NAICS 72) as of 2000. Other startup-intensive sectors include the "other services" sector (which includes businesses like auto repair shops, household maintenance services, drycleaners, laundromats, and funeral homes); professional, scientific, and technical services; and construction. In addition to utilities, sectors with low startup activity include manufacturing, mining, and finance and insurance. These rankings of startup activity are broadly consistent over time and match those reported by (Hurst & Pugsley 2011).

# 4 Econometric Models and Results

This section walks through the econometric models used and the primary results of the paper. We begin by performing standard panel regressions using data from all U.S. counties. We use this basic model to motivate our use of the border discontinuity method. After reporting and discussing the results of the border discontinuity method, we then test for

[10] Using matched data from the Census Bureau's Longitudinal Employer-Household Dynamics and Longitudinal Business Database, (Ouimet & Zarutskie 2014) show that the higher shares of young workers seen among young firms remain even within firm size, industry, and region cells.

two particular sources of bias that may affect our estimates. First, we study the extent to which cross-border spillovers may be driving the results by setting up a model specifically designed to estimate any spillovers. Second, out of concern that states making policy changes may act strategically based on how much of their economic activity occurs on borders, we report results for a subset of states whose border counties comprise a small fraction of their overall economy. Restricting the data in this way assuages concerns that states in which state-border discontinuities would be especially salient are disproportionately likely to cut taxes or maintain low minimum wages. Our robustness checks support the validity of our identification strategy and in certain cases suggest that our results may actually understate rather than overstate the true effects of the policies.

## 4.1 Baseline Panel Regressions

To introduce the results and notation, we first estimate a straightforward panel regression model using all U.S. counties. The corporate tax rate, logged real minimum wage and personal income tax rate variables are each at the state-quarter level, logged population is at the county-quarter level and the outcome variables (logged employment, employment growth, job creation and job destruction) are at the county-quarter level and are as defined above.[11] The specification takes the following form:

$$y_{ct} = \beta_1 CorpTax_{st} + \beta_2 MinWage_{st} + \beta_3 PersTax_{st} + \gamma X_{ct} + \delta_c + \alpha_t + \epsilon_{ct} \quad (1)$$

The coefficients of interest are $\beta_1$, $\beta_2$ and $\beta_3$. $X_{ct}$ represents the set of control variables, $\delta_c$ represents a set of county-level fixed effects to control for time-invariant differences between counties, and $\alpha_t$ is a set of quarter indicator variables that control for common nationwide temporal shocks. Because the policies in question vary at the state level, that is the level at which we cluster standard errors. To maintain consistency throughout the specifications, counties with fewer than 3,000 workers in 2013 are dropped from the sample. Note that with this set of fixed effects, the panel regression is essentially a difference-in-differences estimator.

The results of this model are found in Table 3. Table formats are similar throughout Section 4. Each column gives coefficient results from a separate regression. At the top of the column is the outcome variable used in the regression. The rows list the coefficients on the different policy variables. Columns 1-4 report regression results for "Startup firms". Columns 5-8 report results for the "All Firms" category, which includes startups. The four

[11] Here and below the use of the word "quarter" refers to year-quarter rather than quarter specific seasonal dummies.

outcome variables examined are logged employment, employment growth, job creation and job destruction, where growth, creation and destruction are all scaled as described in Section 3. Column 1 reports the coefficients of the three policies on logged employment in startup firms. The negative and statistically significant coefficient on the corporate tax rate variable suggests that a one-percentage-point increase in the corporate tax rate results in a 4.9 percent drop in the number of workers employed at startup firms. The corporate tax coefficients related to employment growth, job creation and job destruction are not statistically significant. The coefficient on logged employment for "All Firms" is also negative and statistically significant but is less than half the size of the coefficient for startups. Here, a one-percentage-point increase in the corporate tax rate results in a 1.8 percent drop in the number of overall workers. We hold off on interpreting columns 2-4 and 6-8 until later. Some patterns do appear to emerge from the coefficients on the minimum wage and personal income tax rate variables, but they are mostly statistically insignificant and do not reveal any striking conclusions. Given concerns over differences in pre-existing trends and dynamic selection, we now consider a different identification strategy.

Before making too much of these coefficients, it is worth reflecting on the assumptions required for $\beta_1$, $\beta_2$ and $\beta_3$ to be interpreted as the causal impact of corporate tax, minimum wage and personal income tax changes, respectively. As with any difference-in-differences estimator, there should be common trends for both the treated and the untreated observations. States making these policy adjustments should have similar trends to states that do not make adjustments before the policy goes into effect. A related threat to identification is dynamic selection, whereby states make policy adjustments based on past, current or predicted economic activity. If, as discussed above, states only raise minimum wages when labor markets are strong and employment is growing, this will upwardly bias the coefficient on the minimum wage. Another potential concern is that there are unobserved geographic shocks that are correlated with the policy change of interest. These are important reasons standard panel data regression models may result in biased results, and they have been discussed in a number of previous papers on the subject (Dube *et al.* 2010; Ljungqvist & Smolyansky 2014; Meer & West 2016).

## 4.2 Border Discontinuity

Keeping these potential identification problems in mind, we turn to the border discontinuity method which, by exploiting differences in labor market outcomes between contiguous counties that straddle a state border, overcomes many of these concerns. Neighboring counties are likely to experience similar economic conditions and have similar local shocks, but by dint

of falling on one side of a state border, one county will experience the policy shock while its neighbor does not. Even if states adjust policies based on their overall economic conditions and border counties experience similar economic trends as the state, it is still likely that their neighboring, cross-state county will experience similar conditions.

To perform this analysis we create a dataset consisting of all counties that share a border with a county from another state. To understand how we exploit cross-border differences it is useful to first consider the following specification.

$$y_{pct} = \beta_1 CorpTax_{ct} + \beta_2 MinWage_{ct} + \beta_3 PersTax_{ct} + \gamma X_{ct} + \delta_c + \alpha_{pt} + \epsilon_{ct} \tag{2}$$

Here we observe the outcome variable $y_{pct}$ for county $c$ in time period $t$, where county $c$ belongs to county-pair $p$. $\beta_1$, $\beta_2$ and $\beta_3$ are the coefficients of interest and $X_{ct}$ continues to consist of control variables such as logged population that vary at the county-year level. As with the panel specification, $\delta_c$ represents a set of county level fixed effects. What distinguishes this model from the panel model is the inclusion of $\alpha_{pt}$, a set of county-pair-year fixed effects. Inclusion of county-pair-year fixed effects absorbs any shock that is common to a county-pair in a particular period. Importantly, the variation used to identify $\beta_1$, $\beta_2$ and $\beta_3$ is now restricted to changes in within-pair differences.

This specification overcomes the identification concerns inherent in the standard panel regressions but has two shortcomings that require it to undergo a few additional changes. The first (and more pedestrian) issue with equation 2 is that it is computationally intensive. Inclusion of both county and pair-quarter fixed effects requires considerable computational resources. A second issue, and one that may potentially bias the estimates, is that the specification assumes that the bordering county, which serves as the control group, experiences no change in any of the three policy variables or the control variables. Therefore, any change in within-pair differences that is driven by policy changes in the border county will not be attributed to the policy.

To address these concerns we perform a variable transformation similar in spirit to Dube *et al.* (2016) and Hagedorn *et al.* (2015). Consider two contiguous counties, $i$ and $j$, that straddle a state border. For every variable we perform the following transformation.

$$\widetilde{z_{it}} = z_{it} - z_{jt} \tag{3}$$

where $z_{it}$ is the variable in county $i$ in time $t$ and $z_{jt}$ is the variable in the county that borders county i in time t. This transformation automatically captures any period-specific

shock that occurs to any particular pair of counties. We can now rewrite equation (2) in the following way, having transformed each of the variables in the equation to be the within border difference of that variable.

$$\widetilde{y_{it}} = \beta_1 \widetilde{CorpTax_{it}} + \beta_2 \widetilde{MinWage_{it}} + \beta_3 \widetilde{PersTax_{it}} + \Gamma \widetilde{X_{it}} + \delta_i + e_{it} \quad (4)$$

In this equation $\widetilde{y_{it}}$ represents the county-pair difference in the outcome variable (logged employment, employment growth, job creation, job destruction for startups and all firms). $\widetilde{CorpTax_{it}}$ is the difference in the counties' corporate tax rates, $\widetilde{MinWage_{it}}$ is the difference in the counties logged minimum wage, $\widetilde{PersTax_{it}}$ is the difference in the counties' personal tax rates and $\widetilde{X_{it}}$ is the difference in their control variables. $\delta_i$ is a border-specific fixed effect. Any time-invariant difference in economic outcomes between two bordering counties is absorbed through the inclusion of $\delta_i$. By including $\delta_i$ we are now identifying the impact of the policy change off of *changes* in the within-pair differences. Following Dube *et al.* (2016) we also cluster at both the state and the border-segment level.[12]

Table 4 reports results from the locally differenced regression in equation (4). Panel A reports results for specifications that include each of the three policy variables, Panel B includes examines only the corporate tax variable, Panel C includes only the minimum wage variable and Panel D includes only the personal income tax variable. Overall, corporate taxes appear to reduce employment in startup firms and overall employment and they have negative but statistically insignificant effects on employment growth, job creation and job destruction. There is little impact of minimum wage increases on employment *levels* in startup firms; however, there are relatively large negative effects on employment growth, negative effects on job creation and positive effects on job destruction in startup firms. Personal income tax rates have no clear impacts on either startup firm outcomes or "All Firm" outcomes.

To make sense of these results it is necessary to closely consider the economic significance of the coefficients and their relationship to one another. To begin, it is important to note that, despite the variable transformation, the coefficients from Table 4 have the same interpretation as those in the panel estimates reported in Table 3. Columns 1-4 focus on the effect of the policies on startups. Panel A shows a coefficient on the corporate tax variable of -3.613. To interpret this coefficient we can consider what this would imply for a one-percentage-point change in a state's corporate tax rate. As can be seen in Figure 1, a one-percentage-point change would be a large but not unprecedented change over the time period we are

[12]Counties enter the dataset as many times as they have a border pair in a contiguous state; as a result there may be correlation across both states and border-segments. To implement this we use stata's reghdfe command Correia (2014) which allows for two-way clustering of standard errors following Cameron *et al.* (2011)

examining. According to the coefficient in column 1 of Panel A, a one-percentage-point increase in the corporate tax rate would reduce employment in startup firms by 3.6 percent. Interestingly, despite the differing sources of variation, the corporate tax results in Table 3 are similar to those in Table 4. As with Table 3, the corporate tax coefficients in columns 2-4 are all statistically insignificant, but their signs suggest that employment growth, job creation and job destruction in startup firms all decline.[13] Columns 5-8 examine the impact on "All Firm" outcomes. The coefficient in column 5 implies that a one-percentage-point increase in corporate taxes reduces overall employment by 1.4 percent. Importantly, the employment effect on all firms is smaller than the effect on startup firms. Past research has shown that startups are more sensitive to negative shocks than older firms (Fort *et al.* 2013). This also suggests that a disproportionately large share of the overall employment loss is occurring at new firms and that increases in corporate taxes are hampering entrepreneurial activity.

In this specification the employment growth, job creation and job destruction coefficients are not statistically significant, but they are in some other specifications so it is worthwhile to interpret their magnitudes and understand the relationship the coefficients have to each other. As discussed in section 3, employment change, job creation and job destruction for startup firms are all scaled by *total* firm employment in the county. By scaling both the "startup firm" variables and the "all firm" variables by the same number, we are able to directly compare coefficients across firm age groupings, as is done later in the paper, and understand the portion of the impact on overall employment growth change that is attributable to the impact on startups (note that in some literature, these measures of employment growth, job creation and job destruction are described as "components" of overall flows).[14] The -0.131 coefficient in the first row of column 6 of Table 4 can be interpreted to mean that a one-percentage-point increase in a state's corporate tax rate results in a 0.13 percent decline in their quarterly employment growth rate. At first glance this may seem small. However, a decline in the quarterly growth rate of this magnitude can lead to substantial levels changes after a few years. Both creation rates and destruction rates decline as well, suggesting that corporate tax rate increases lead to declines in overall job reallocation across firms.

Because we have scaled startup employment growth, job creation and job destruction

[13]Note that because of the employment change identity ($\Delta Emp_t = Emp_t - Emp_{t-1} = Creation_t - Destruction_t$) the sign and magnitude of the employment growth coefficient in column 2 approximates the job creation coefficient minus the job destruction coefficient.

[14]In Table A2 we report results for all five of the firm age groupings provided by the QWI as well as for the "all firm" category. Adding the employment growth coefficients for each of the firm age groups will equal the overall employment growth coefficient. The same holds for the job creation coefficients and the job destruction coefficients. In practice the arithmetic is not exact, since our county size thresholds result in slightly differeing samples.

by the same factor as overall employment growth, job creation and job destruction, we can gain real insight into the extent that the coefficients on the "All Firms" columns are being driven by changes in startup employment growth, job creation and job destruction. A coefficient of -0.0457 on startup employment growth implies that an outsized portion of the overall employment change coefficient (-0.131) is accounted for by employment change in startups. Roughly one-fifteenth of overall employment is located in startup firms. Therefore, if employment growth were equally impacted across firm age groupings then the startup employment growth coefficient would be one-fifteenth the size of the overall employment growth coefficient. Instead it is more than one-third the size of the overall coefficient.

Although scaling both the "Startup" firm results and the "All Firm" variables by the same factor allows for direct comparison, it has the disadvantage of not permitting an easy interpretation of the startup growth, creation and destruction regressions. To interpret the startup coefficients as rates we would need to multiply them by a factor of roughly fifteen, as startup firms account for about one-fifteenth of overall employment (see Table 1).

Given that they are not strictly statistically significant, it may seem unnecessary to dwell on the interpretation of these particular corporate tax rate coefficients. However, other specifications and the minimum wage results suggest that there are in fact statistically significant effects of policies on these outcomes.

Consider the minimum wage results in row 2. There is no impact of the minimum wage on logged employment for startup firms, but there are large effects of the minimum wage on startups' employment growth, on their job creation and on their job destruction. The employment growth coefficient of -0.00206 can be interpreted to mean that a 10 percent increase in the minimum wage reduces quarterly employment growth in startups by .003 percent (see Figure 2 for historical minimum wage changes, many of which are close to or above 10 percent). Job creation rates in startups falls and job destruction rates increase. This is consistent with results by Meer & West (2016) who find minimum wage increases affect employment growth but have minimal effect on employment levels in difference-in-differences frameworks.[15]

Given that many firms may choose to organize as S-corporations, LLCs, sole proprietorships or partnerships, it is important to examine the effect of personal income tax rates as well. Perhaps surprisingly, we find minimal impact of personal income taxes on employer startups. As seen in Table A2, other firm age categories, including 2-3 year old firms, appear

[15] A zero logged employment finding can be consistent with a negative employment growth finding if treated counties are trending upward relative to their counterfactual prior to the treatment. Figure A1 demonstrates an employment path for a treated county for which a difference-in-differences estimation will find zero effect on logged employment but strong negative effects for employment growth.

to respond more strongly to changes in personal income tax rates. There are statistically significant negative coefficients for the creation and destruction coefficients for "All Firms',' but the lack of a strong result on startups remains surprising.

### 4.3 Border Spillovers

A primary concern with border discontinuity models is that they may overstate the size of the treatment effect if the control county is subject to spillovers from the treated county. In the context of our design, there is concern that increases in taxes or minimum wages may result in startups simply choosing to locate on the other side of the state border. If new firms react in this way then border discontinuity methods will find large negative impacts of the policy when in fact there is (possibly) zero net change to entrepreneurial activity. Of course, negative spillovers may occur as well, whereby a negative shock to one county reduces rather than increases economic activity in bordering counties. Fortunately, the direction of any economic spillover can be directly tested in the data. We run the following model on a dataset that includes all U.S. counties.

$$y_{ct} = \phi_1 \overline{CorpTax}_{ct} + \phi_2 \overline{MinWage}_{ct} + \phi_3 \overline{PersTax}_{ct} + \gamma X_{ct} + \delta_c + \alpha_{st} + \epsilon_{ct} \tag{5}$$

For U.S. counties that border another state, the variables $\overline{CorpTax}_{ct}$, $\overline{MinWage}_{ct}$ and $\overline{PersTax}_{ct}$ are set equal to the corporate tax, minimum wage and personal tax rate of the bordering state. For all interior counties, these variables are set equal to zero. The model includes the same set of control variables, $X_{ct}$, as well as $\delta_c$, a full set of county fixed effects. Crucially, the model also contains a full set of state-quarter fixed effects, represented by $\alpha_{st}$. This set of fixed effects absorbs any time-specific shock that is common to all counties in a state. Because state policies vary at the state-quarter level, these fixed effects also absorb any own-state effect that our policies of interest, or any other state level policies, may have.

The coefficients $\phi_1$, $\phi_2$ and $\phi_3$ capture any spillovers that border counties may experience from their neighboring states' policies. The variation that identifies these spillovers comes from changes in within-state differences between border and interior counties that coincides with changes in neighboring states' policies.

Table 5 reports results from equation (5). The coefficients in this model can be interpreted in the same manner as the coefficients in Table 4. Coefficients on the $\overline{CorpTax}_{ct}$ coefficient generally point negative and are not statistically significant. The negative signs on the logged employment specification and the employment growth specification suggest that firms did

not relocate across the borders in large enough numbers to be measureable. The negative signs suggests that bordering counties were potentially negatively affected by neighboring state policies. If this is the case then the border discontinuity results may slightly *understate* the true size of the policies' impact.

In regards to minimum wage, there is some evidence that border spillovers may be leading us to overstate the impact of minimum wages on entrepreneurship and that young firms are choosing to locate in nearby counties. The coefficients on logged employment, employment growth and job creation are positive and borderline statistically significant. The magnitude of the coefficient in the employment growth specification suggests the minimum wage border discontinuity estimates in Table 4 may overstate the impact of minimum wage on startup employment growth. Compare the minimum wage coefficient of -0.00206 in Table 4 to the spillover coefficient of 0.000843 in Table 5. The border discontinuity method assumes that bordering counties experience zero effect from the policy and are valid controls. However, if minimum wages positively impact growth in the bordering county, that could signal that firms are relocating to nearby areas with cheaper cheaper labor costs. If two bordering counties have the same number of new firm workers prior to the minimum wage increase, then a 1% increase in the bordering county would result in border discontinuity finding of negative 2% even though there was zero net change.[16] Therefore, the spillover coefficient only needs to be half the size of the border discontinuity coefficient in order to imply full reallocation of new firms to the bordering county and zero net effect of minimum wages on entrepreneurship. In this case, a spillover coefficient of 0.000843 implies that 75% of the border discontinuity coefficient can be accounted for by the presence of spillovers.

There is no evidence of minimum wage changes spilling over to impact the "All Firm" results. This is not particularly surprising given that new firm employment is likely to be more mobile. Personal income tax appears to have little effect on bordering counties. Job creation and destruction results for 'All Firms' are negative but are far smaller in magnitude than the creation and destruction coefficients for the border discontinuity results in Table 4.

Overall, the test for border spillovers suggest that startup activity does not simply shift across the border in response to corporate tax changes or personal income tax changes. There is, however, evidence that shifting in response to the minimum wage may be responsible for some of the minimum wage results. We take this into consideration when discussing the internal validitiy of the results.

[16]This assumes that both counties have equal employment before the policy change and that the one percent increase in the bordering county is the result of one decrease in the policy-affected county.

## 4.4 Robustness Checks

To examine the sensitivity of our results, we provide a variety of robustness checks. One potential concern is that state governments may be aware of the degree to which changes in their policies will affect startups and drive employment to their state. In other words, states in which state-border discontinuities would be especially salient might be disproportionately likely to cut taxes or maintain low minimum wages. If true, then we may not be able to generalize our results to all states. While there is no direct way to test for this, we can restrict our sample to only states whose border counties make up a relatively small fraction of their overall activity. States with a low share of economic activity on their borders are less likely to consider the potential impact on border counties when making policy decisions. The orange shaded region in Figure 4 represent counties that belong to states for whom greater than 50 percent of overall state employment is located in a border county. Regression results that exclude these counties are reported in Table 6. The key coefficients are very similar to the baseline results found in Table 4.

A variety of other robustness checks are also included in the appendix. Table A1 reports results where linear state trends are included in the model. The inclusion of these trends has been hotly debated in the minimum wage literature (Neumark *et al.* 2014). Their inclusion will absorb some of the treatment effect if the policy results in a shift in both trends and levels. Not surprisingly, results from Table A1 show that inclusion of these trends does in fact reduce the magnitude of some of the coefficients. However, while their size is smaller the overall story is little changed. Table A3 requires that counties on each side of the border have over 1,000 workers (instead of 3,000 workers as in other specifications). Results are not sensitive to changing this threshold. Table A4 specifically examines the corporate tax results by limiting the data to only the three-year periods surrounding corporate tax changes in each state. Again results are similar to the baseline results. Table A5 drops observations flagged by the QWI as having undergone significant distortion. The QWI distorts some values so as to prevent disclosure of any single establishment's employment or growth. Again, results are similar to the baseline.

Finally, we report results which break down the effect of these policies by sector. In addition to county-firm age data, the QWI reports data at the county-firm age-sector level. Table A6 examines the impact on the thirteen largest sectors in the data. Before discussing these results, an important caveats bears mention. First, the county-firm age-sector data are far more likely to be suppressed due to the small number of firms and workers in these categories. Sectors with fewer than 10,000 observations provided particularly noisy estimates and are not reported. Nonetheless, we believe that these results provide suggestive evidence

for how policy changes may differentially impact sectors in the economy. Businesses operating in accomodation and food services, finance and insurance, and retail trade appear to be particularly responsive to changes in policies.[17] As discussed above, accomodation and food services is a particularly startup-intensive sector with almost 10 percent of its employment being accounted for by startups, while retail trade lies in the middle of the ranking at about 4 percent. Finance and insurance began the 2000's with just over 3 percent of its employment being accounted for by startups, but by 2014 this share had fallen almost to 1 percent.

# 5 Discussion

Taken together, the results discussed above suggest priorities for researchers and policymakers in the realm of entrepreneurship. First, a comparison of the simple panel regression approach with our border discontinuity design suggests the importance of finding plausibly exogenous sources of policy variation of relevance to entrepreneurial activity. In the case of corporate taxes, the panel regressions produce larger estimates of the negative effect of taxes on entrepreneurial activity than do the border discontinuity regressions. It is likely that an important cause of this difference is endogeneity of tax policy to economic conditions. Additionally, however, there may be important differences between border and interior counties that drive the results. One might suppose that interior counties are less sensitive to corporate tax increases since entrepreneurs in these counties may face higher costs of relocating business activity to neighboring states. But our results on spillovers suggest that simple cross-border movements of activity are not likely to be a main driver of our border discontinuity estimates for corporate taxes.

Consistent with the growing literature on young firm activity, our results indicate that new firms are particularly vulnerable to economic shocks. New firms account for a disproportionate share of the overall response of employment growth, job creation and job destruction to changes in corporate tax rates. New firms are also the most likely to destroy jobs in the wake of minimum wage increases (though, interestingly, the job creation response of new firms is minimal while that of older firms is significant). In results discussed in the appendix (Table A2) we find that startup activity is particularly vulnerable even compared to other young firms. It appears that, consistent with related research, the firm entry margin is crucial for understanding broader employment dynamics.

Our estimates of the effects of personal tax rates on economic activity do not point in clear

[17] While they are not statistically significant, coefficients on manufacturing and health care are positive. This lack of a strong negative coefficient in manufacturing assuages potential concerns that our results are driven by large foreign-owned manufactures building facilities deciding to locate production in the United States.

directions. Among all firms, increases in personal tax rates dampen reallocation through reductions in both job creation and job destruction, but reallocation effects for entrants are not significant. These results differ from past work on the subject of entrepreneurship and personal tax rates (Gentry & Hubbard 2000), which examine proxy for entrepreneurship with entry into self-employment. In the QWI, firms are only observed in the data when they hire their first worker. This margin may respond differently to personal tax rates than would entry into self-employment. Given the lack of direct effects of personal tax rates on new firms, it is also possible that important general equilibrium mechanisms are relevant and may deserve further investigation.

Various measures of entrepreneurship have declined nationally and within states during the time period we study. In one sense, our results deepen the puzzles behind those declines. We provide evidence that tax increases reduce entrepreneurial activity, but state and federal corporate tax rates have generally fallen in recent decades in the U.S. Barring strong national general equilibrium mechanisms, our results suggest that entrepreneurial activity would have declined even more in the absence of widespread tax rate reductions. Still, the results point to important policy dilemmas for policymakers focused on fostering entrepreneurship.

# 6 Conclusion

We provide estimates of the effects of changes in corporate tax rates, minimum wages and personal tax rates on entrepreneurial activity and employment generally. Notably, ours is the first study to investigate the effect of these state policies on entrepreneurial activity using plausibly exogenous policy variation. We find significant effects of corporate tax rates on aggregate employment, with intensified effects on employment at new firms—again highlighting the importance of the firm entry margin for broader economic dynamics. Effects of corporate tax rates on employment growth rates and gross job flows are statistically insignificant but point to theoretically plausible effects, consistent with estimated employment level effects, that could be substantial over long periods of time. Consistent with existing literature, minimum wage changes from past observed levels have only moderate effects on economic activity (and, we show, are likely subject to spillover concerns); new firms play an interesting role in these effects, accounting for almost half of the overall effect on employment growth and a larger share of job destruction but with no measured level effects. Changes in personal tax rates have little effect on business activity, but they do act to reduce gross flows.

Our results are interesting and useful in their own right as they inform researchers and policymakers about the aggregate consequences of various policies for business activity gen-

erally and young firm activity in particular. However, future work should augment these results by exploiting additional sources of variation. As mentioned previously, most states treat C-corporations differently from S-corporations, LLCs, sole proprietorships and partnerships for tax purposes. This suggests that these different types of firms should respond differently to changes in corporate tax rates. Industry variation would also be useful as a robustness check as well as a means of understanding effect heterogeneity and cross-border spillovers. Even more variation can be obtained through the use of a longer time series of firm dynamics data. These added investigations require detailed microdata; however, our current results using newly available public data from the QWI innovate significantly on the existing entrepreneurship literature.

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# Figures and Tables

Figure 1: Corporate tax changes

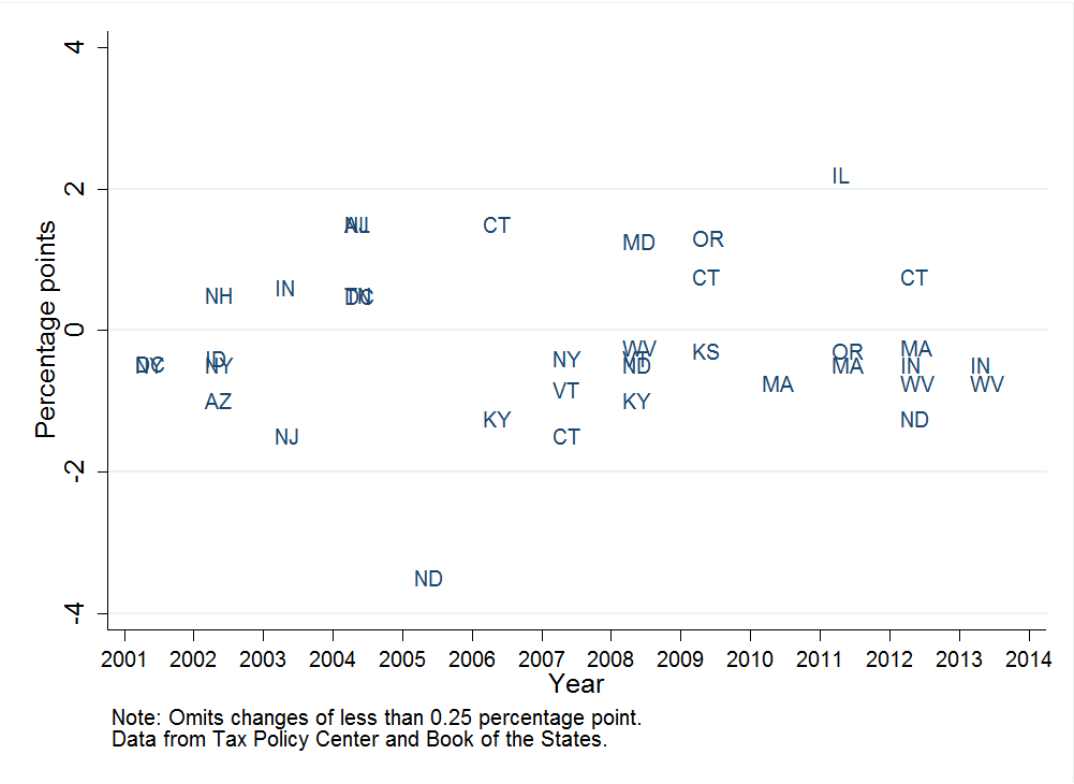


Figure 2: Minimum wage changes

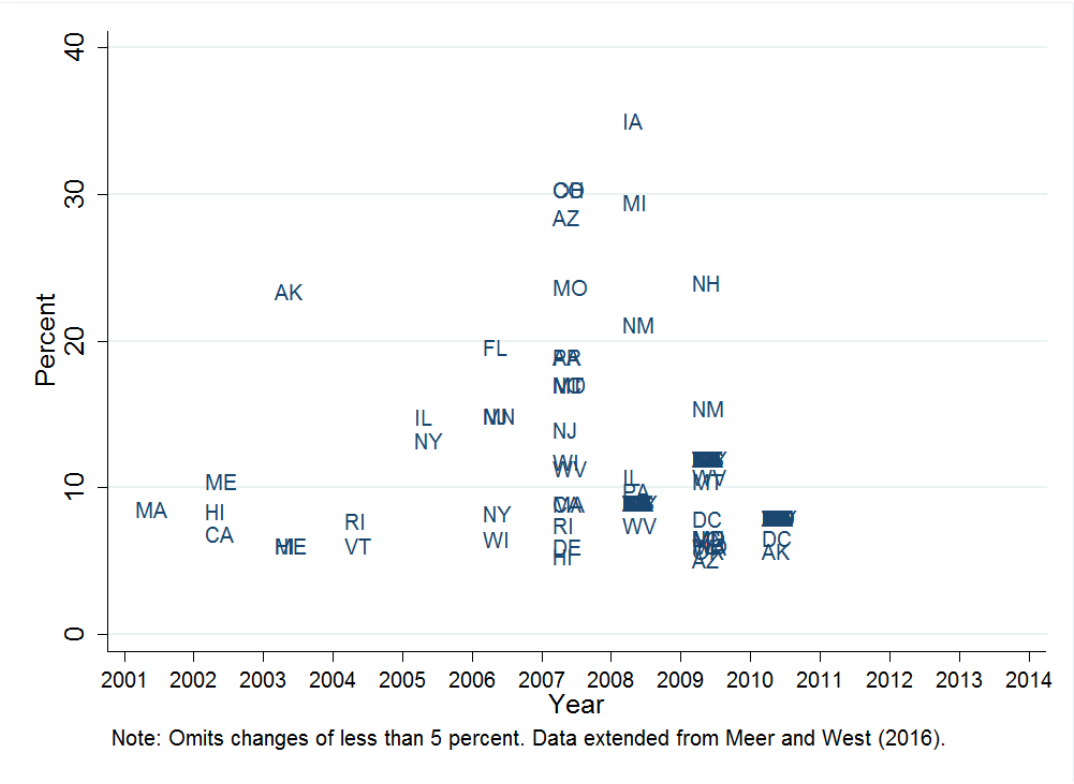


Figure 3: Personal tax changes

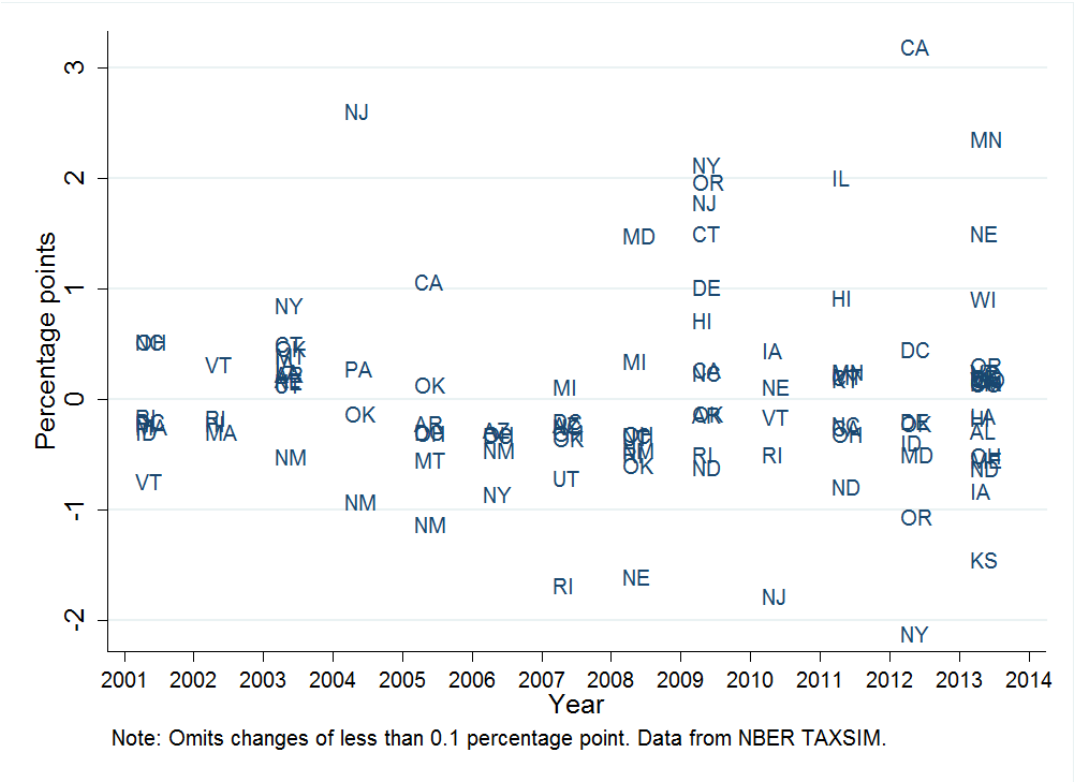
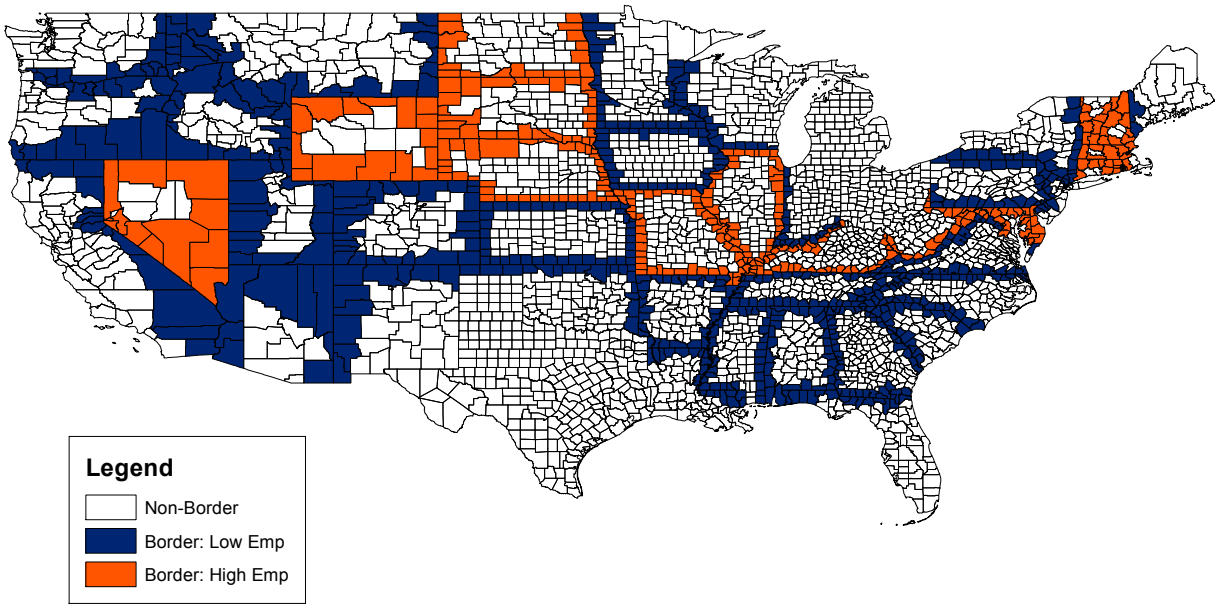


Figure 4: Map of Border Counties in Sample




Note: The above figure shows the counties that are in our border sample. The counties in orange are the border counties belonging to states for whom more that 50% of their employment is located in a border county.

Table 1: Summary Statistics

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | All Firms All Cntys | All Firms Border Cntys | New Firms All Cntys | New Firms Border Cntys |
| Employment | 42,535 | 36,780 | 2,388 | 1,938 |
| | (161,436) | (136,036) | (10,061) | (6,922) |
| % Total Employment | | | 0.0646 | 0.0643 |
| | | | (0.0399) | (0.0395) |
| Avg. Monthly Earn | 2,563 | 2,549 | 1,892 | 1,872 |
| | (800) | (790) | (795) | (846) |
| Creation Rate | 0.0603 | 0.0611 | 0.2029 | 0.2134 |
| | (0.0200) | (0.0200) | (0.1070) | (0.1174) |
| Destruction Rate | 0.0584 | 0.0592 | 0.1113 | 0.1189 |
| | (0.0209) | (0.0203) | (0.1416) | (0.1492) |
| New Hires Avg. Monthly Earn | 1,685 | 1,689 | 1,571 | 1,557 |
| | (578) | (592) | (905) | (786) |
| Counties | 3,128 | 1,135 | 3,128 | 1,135 |
| Observations | 213,223 | 76,777 | 213,223 | 76,777 |

Note: The above table provides summary statistics for all counties, border counties, all firms and new firms. Border counties are shown to be slightly smaller on average. New firms comprise roughly 6.4% of employment for all counties and for border counties.

Table 2: State Summary Statistics

| State | (1) % Emp in Young Firms | (2) Creation Rate | (3) Young Firm Creation Rate | (4) Employment |
|---|---|---|---|---|
| Alabama | 0.0361 | 0.0446 | 0.0062 | 1,831,758 |
| Alaska | 0.0335 | 0.0694 | 0.0072 | 296,528 |
| Arizona | 0.0404 | 0.0588 | 0.0075 | 2,590,792 |
| Arkansas | 0.0356 | 0.0444 | 0.0063 | 1,150,874 |
| California | 0.0508 | 0.0525 | 0.0092 | 14,847,841 |
| Colorado | 0.0440 | 0.0772 | 0.0095 | 2,291,857 |
| Connecticut | 0.0312 | 0.0428 | 0.0054 | 1,641,621 |
| Delaware | 0.0326 | 0.0571 | 0.0061 | 425,409 |
| District of Columbia | 0.0236 | 0.0660 | 0.0053 | 548,778 |
| Florida | 0.0503 | 0.0658 | 0.0093 | 7,921,902 |
| Georgia | 0.0404 | 0.0516 | 0.0073 | 3,962,365 |
| Hawaii | 0.0375 | 0.0426 | 0.0062 | 538,555 |
| Idaho | 0.0529 | 0.0603 | 0.0101 | 573,081 |
| Illinois | 0.0342 | 0.0435 | 0.0057 | 5,837,481 |
| Indiana | 0.0317 | 0.0470 | 0.0051 | 2,969,536 |
| Iowa | 0.0317 | 0.0452 | 0.0053 | 1,451,346 |
| Kansas | 0.0360 | 0.0524 | 0.0068 | 1,292,493 |
| Kentucky | 0.0303 | 0.0535 | 0.0055 | 1,780,521 |
| Louisiana | 0.0405 | 0.0722 | 0.0080 | 1,929,170 |
| Maine | 0.0384 | 0.0544 | 0.0070 | 585,597 |
| Maryland | 0.0363 | 0.0661 | 0.0073 | 2,362,169 |
| Michigan | 0.0343 | 0.0518 | 0.0060 | 4,170,887 |
| Minnesota | 0.0314 | 0.0592 | 0.0058 | 2,666,734 |
| Mississippi | 0.0355 | 0.0522 | 0.0065 | 1,060,738 |
| Missouri | 0.0374 | 0.0448 | 0.0062 | 2,682,996 |
| Montana | 0.0511 | 0.0677 | 0.0103 | 395,561 |
| Nebraska | 0.0342 | 0.0417 | 0.0057 | 885,474 |
| Nevada | 0.0494 | 0.0518 | 0.0086 | 1,235,014 |
| New Hampshire | 0.0313 | 0.0457 | 0.0055 | 623,099 |
| New Jersey | 0.0352 | 0.0571 | 0.0072 | 3,900,524 |
| New Mexico | 0.0473 | 0.0684 | 0.0088 | 756,346 |
| New York | 0.0380 | 0.0540 | 0.0070 | 8,395,378 |
| North Carolina | 0.0371 | 0.0524 | 0.0070 | 3,948,814 |
| North Dakota | 0.0386 | 0.0527 | 0.0069 | 336,391 |
| Ohio | 0.0298 | 0.0440 | 0.0048 | 5,318,822 |
| Oklahoma | 0.0414 | 0.0546 | 0.0074 | 1,469,287 |
| Oregon | 0.0413 | 0.0524 | 0.0078 | 1,596,845 |
| Pennsylvania | 0.0309 | 0.0511 | 0.0054 | 5,747,465 |
| Rhode Island | 0.0343 | 0.0514 | 0.0063 | 471,790 |
| South Carolina | 0.0413 | 0.0517 | 0.0074 | 1,833,548 |
| South Dakota | 0.0398 | 0.0514 | 0.0069 | 373,321 |
| Tennessee | 0.0346 | 0.0461 | 0.0059 | 2,760,668 |
| Texas | 0.0454 | 0.0494 | 0.0079 | 9,830,599 |
| Utah | 0.0488 | 0.0560 | 0.0089 | 1,163,243 |
| Vermont | 0.0338 | 0.0571 | 0.0063 | 296,935 |
| Virginia | 0.0346 | 0.0596 | 0.0065 | 3,544,420 |
| Washington | 0.0443 | 0.0533 | 0.0088 | 2,722,032 |
| West Virginia | 0.0347 | 0.0484 | 0.0061 | 674,684 |
| Wisconsin | 0.0314 | 0.0431 | 0.0054 | 2,642,325 |
| Wyoming | 0.0481 | 0.0722 | 0.0098 | 262,452 |
| | Avg | Avg | Avg | Total Emp |
| | 0.0381 | 0.0542 | 0.0070 | 128,596,062 |

Note: The state level summary statistics are calculated using the QWI. Massachusetts is excluded from the data as it did not join the QWI until 2010.

Table 3: Base Panel Regressions, All Counties

| | Results: 0-1 Year Old Firms | | | | Results: All Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
| **Corporate Taxes, Minimum Wage, Personal Taxes** | | | | | | | | |
| **Corp Rate** | -4.876** | -0.0496 | -0.0686 | -0.0191 | -1.807** | -0.135 | -0.169 | -0.0344 |
| | (2.363) | (0.0358) | (0.0531) | (0.0175) | (0.811) | (0.129) | (0.239) | (0.118) |
| **Min Wage** | -0.113 | -0.000232 | -0.000490 | -0.000258 | 0.00297 | 0.000378 | -0.00530 | -0.00566 |
| | (0.158) | (0.000591) | (0.000858) | (0.000461) | (0.0299) | (0.00341) | (0.00402) | (0.00355) |
| **Personal Rate** | 0.142 | -0.00424 | -0.0104 | -0.00615** | 0.166 | 0.0421 | -0.0479 | -0.0901** |
| | (0.854) | (0.00746) | (0.00956) | (0.00270) | (0.751) | (0.0386) | (0.0340) | (0.0357) |
| Observations | 115,627 | 115,190 | 115,190 | 115,190 | 115,953 | 115,403 | 115,403 | 115,403 |
| $R^2$ | 0.934 | 0.065 | 0.171 | 0.171 | 0.996 | 0.098 | 0.288 | 0.278 |

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$
Note: These results are obtained from a basic differences-in-differences model where both border and non-border U.S. counties are included.

Table 4: Border Discontinuity Results

| | Results: 0-1 Year Old Firms | | | | Results: All Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
| **Panel A: Corporate Taxes, Minimum Wage, Personal Taxes** | | | | | | | | |
| **Corp Rate** | -3.613** | -0.0457 | -0.0633 | -0.0177 | -1.416** | -0.131 | -0.158 | -0.0278 |
| | (1.319) | (0.0296) | (0.0398) | (0.0116) | (0.461) | (0.0938) | (0.136) | (0.0493) |
| **Min Wage** | 0.0123 | -0.00206** | -0.000909 | 0.00115** | -0.0645** | -0.00490* | -0.00488* | -0.00000342 |
| | (0.0990) | (0.000708) | (0.000897) | (0.000486) | (0.0227) | (0.00284) | (0.00288) | (0.00241) |
| **Personal Rate** | -0.291 | 0.00675 | 0.00674 | 0.000353 | -0.340 | -0.0234 | -0.103** | -0.0801** |
| | (0.909) | (0.0105) | (0.0136) | (0.00565) | (0.428) | (0.0270) | (0.0408) | (0.0281) |
| Observations | 51,106 | 51,106 | 51,106 | 51,106 | 51,258 | 51,258 | 51,258 | 51,258 |
| $R^2$ | 0.910 | 0.072 | 0.224 | 0.091 | 0.996 | 0.025 | 0.214 | 0.191 |
| **Panel B: Corporate Taxes Alone** | | | | | | | | |
| **Corp Rate** | -3.634** | -0.0457 | -0.0630 | -0.0174 | -1.463** | -0.134 | -0.168 | -0.0346 |
| | (1.320) | (0.0302) | (0.0408) | (0.0118) | (0.462) | (0.0956) | (0.139) | (0.0502) |
| Observations | 51,106 | 51,106 | 51,106 | 51,106 | 51,258 | 51,258 | 51,258 | 51,258 |
| $R^2$ | 0.910 | 0.072 | 0.224 | 0.090 | 0.996 | 0.025 | 0.214 | 0.190 |
| **Panel C: Minimum Wage Alone** | | | | | | | | |
| **Min Wage** | -0.00146 | -0.00222** | -0.00113 | 0.00109** | -0.0704** | -0.00543* | -0.00567* | -0.000253 |
| | (0.0994) | (0.000778) | (0.00102) | (0.000505) | (0.0233) | (0.00298) | (0.00320) | (0.00240) |
| Observations | 51,106 | 51,106 | 51,106 | 51,106 | 51,258 | 51,258 | 51,258 | 51,258 |
| $R^2$ | 0.910 | 0.070 | 0.220 | 0.090 | 0.996 | 0.024 | 0.213 | 0.190 |
| **Panel D: Personal Taxes Alone** | | | | | | | | |
| **Personal Rate** | -0.704 | 0.00113 | -0.000704 | -0.00148 | -0.514 | -0.0394 | -0.122** | -0.0833** |
| | (0.922) | (0.0131) | (0.0174) | (0.00623) | (0.434) | (0.0354) | (0.0536) | (0.0303) |
| Observations | 51,106 | 51,106 | 51,106 | 51,106 | 51,258 | 51,258 | 51,258 | 51,258 |
| $R^2$ | 0.910 | 0.070 | 0.220 | 0.090 | 0.996 | 0.024 | 0.213 | 0.191 |

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Note: Above are the main border discontinuity results from equation 999 in the paper. The unit of observation is a county-border with all variables defined as the difference between the two counties that share the border, as seen in equation 999. Border pair fixed effects are included to account for time invariant differences between the two counties. Any shock that occurs to a border pair in a particular quarter is absorbed through this differencing method.

Table 5: Test for Border County Spillovers

| | Results: 0-1 Year Old Firms | | | | Results: All Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
| **Corporate Taxes, Minimum Wage, Personal Taxes** | | | | | | | | |
| **Corp Rate** | -0.569 | -0.00557 | -0.00706 | -0.00163 | 0.279 | -0.0217* | -0.0181 | 0.00373 |
| **Border** | (0.367) | (0.00367) | (0.00441) | (0.00182) | (0.210) | (0.0120) | (0.0151) | (0.0128) |
| **Min Wage** | 0.0567 | 0.000843* | 0.000959* | 0.000112 | 0.00568 | 0.000831 | 0.00396** | 0.00311** |
| **Border** | (0.0406) | (0.000443) | (0.000554) | (0.000218) | (0.0163) | (0.00132) | (0.00161) | (0.00111) |
| **Personal Rate** | -0.0791 | -0.00263 | -0.00295 | -0.000273 | -0.0901 | -0.000355 | -0.0150** | -0.0146** |
| **Border** | (0.206) | (0.00183) | (0.00229) | (0.00101) | (0.0775) | (0.00604) | (0.00678) | (0.00527) |
| State-Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 115,593 | 115,157 | 115,157 | 115,157 | 115,919 | 115,370 | 115,370 | 115,370 |
| $R^2$ | 0.938 | 0.115 | 0.224 | 0.207 | 0.997 | 0.237 | 0.395 | 0.383 |

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Note: This table reports results from regressions that test whether border counties experience spillovers from policy changes that happen in adjacent states. The regressions include both border and interior county observations. For counties that border a neighboring state the variable "Corp Rate Border" is equal to the Corporate Tax Rate of the neighboring state. That variable is set equal to zero for all interior counties. "Min Wage Border" and "Personal Rate Border" are defined in the same way. All specifications include county and year-qtr fixed effects.

Table 6: Dropping Border Dominated States

| | Results: 0-1 Year Old Firms | | | | Results: All Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
| **Corporate Taxes, Minimum Wage, Personal Taxes** | | | | | | | | |
| **Corp Rate** | -5.155*** | -0.0327 | -0.0536 | -0.0209 | -1.974*** | -0.0869 | -0.130 | -0.0430 |
| | (1.543) | (0.0325) | (0.0432) | (0.0145) | (0.588) | (0.0984) | (0.150) | (0.0631) |
| **Min Wage** | 0.0146 | -0.00280** | -0.00132 | 0.00147** | -0.0783** | -0.00592 | -0.00316 | 0.00275 |
| | (0.112) | (0.000853) | (0.00102) | (0.000571) | (0.0244) | (0.00370) | (0.00321) | (0.00315) |
| **Personal Rate** | 0.704 | 0.0170 | 0.0217 | 0.00514 | -0.629 | 0.00249 | -0.0891** | -0.0920** |
| | (1.198) | (0.0119) | (0.0148) | (0.00743) | (0.591) | (0.0288) | (0.0395) | (0.0353) |
| Observations | 36,408 | 36,408 | 36,408 | 36,408 | 36,490 | 36,490 | 36,490 | 36,490 |
| $R^2$ | 0.913 | 0.054 | 0.229 | 0.082 | 0.996 | 0.015 | 0.208 | 0.192 |

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Note: This table reports results using the same border discontinuity method reported in Panel A of Table 4 but drop any state where more than 50% of employment is located in counties that border other states. If border counties comprise the majority of employment in a state then a state's policy decision are less likely to be exogenous to the economic conditions of it's border counties.

# A Appendix

The primary data source used in the paper is the Quarterly Workforce Indicators data which was downloaded from Cornell's Economics Compute Cluster Organization. Corporate tax rate data are obtained from the Tax Foundation and supplemented with data from the Book of States. We use the top corporate tax rate, though the top bracket varies from state to state. States also differ in how they determine the amount of a firm's economic activity that is located in their state, though location of a firm's employment is key part of this determination. Minimum wage data is largely based on the file provided by Meer and West (2016) but is extended using the Department of Labor's State Minimum Wage Report which can be found at `https://www.dol.gov/whd/state/stateMinWageHis.htm`. Personal tax rate data is obtained from NBER's Taxsim program which reports maximum state tax rates by year. These tax rates assume income of $1.5 million and include a variety of local tax policies such as the mortgage intererst deduction. These rates will not perfectly reflect the rates faced by all potential entrepreneurs but serve as a proxy for differences in personal tax rates that households face across geography and across time. More details on the data will be available in future versions of the paper.

Table A1: Including State-Specific Trends

| | Results: 0-1 Year Old Firms | | | | Results: All Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
| **Corporate Taxes, Minimum Wage, Personal Taxes** | | | | | | | | |
| **Corp Rate** | -2.187 | -0.0183 | -0.0259 | -0.00804 | -0.665 | -0.0174 | -0.0189 | -0.00148 |
| | (1.422) | (0.0172) | (0.0228) | (0.00866) | (0.445) | (0.0545) | (0.0800) | (0.0410) |
| **Min Wage** | -0.0934 | -0.00246*** | -0.00143 | 0.00103** | -0.0372* | -0.00556* | -0.00576** | -0.000201 |
| | (0.101) | (0.000726) | (0.000883) | (0.000500) | (0.0192) | (0.00304) | (0.00270) | (0.00270) |
| **Personal Rate** | -0.553 | 0.00208 | 0.00138 | -0.00103 | -0.0168 | 0.000972 | -0.0311 | -0.0324 |
| | (1.159) | (0.0103) | (0.0136) | (0.00562) | (0.320) | (0.0315) | (0.0403) | (0.0300) |
| Observations | 51,106 | 51,139 | 51,106 | 51,106 | 51,258 | 51,258 | 51,258 | 51,258 |
| $R^2$ | 0.911 | 0.082 | 0.247 | 0.095 | 0.996 | 0.030 | 0.233 | 0.195 |

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

This table reports results using the same border discontinuity method reported in Panel A of Table 4 but now includes state specific linear trends. As discussed in prior empirical literature, including these state-specific trends is likely to absorb some of the treatment effect. Whether to include these trends is debated considerably in the minimum wage literature as inclusion of the trends may absorb some of the treatment effect.

Table A2: Results by Firm Age Group

| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
|---|---|---|---|---|---|---|---|---|
| | **Results: 0-1 Year Old Firms** | | | | **Results: 2-3 Year Old Firms** | | | |
| **Corp Rate** | -3.613** | -0.0457 | -0.0633 | -0.0177 | -1.446 | 0.00259 | -0.0120 | -0.0145 |
| | (1.319) | (0.0296) | (0.0398) | (0.0116) | (1.268) | (0.00536) | (0.0137) | (0.00937) |
| **Min Wage** | 0.0123 | -0.00206** | -0.000909 | 0.00115** | 0.0371 | -0.000128 | -0.000163 | -0.0000424 |
| | (0.0990) | (0.000708) | (0.000897) | (0.000486) | (0.0999) | (0.000438) | (0.000488) | (0.000433) |
| **Personal Rate** | -0.291 | 0.00675 | 0.00674 | 0.000353 | -3.016** | -0.00325 | -0.0155** | -0.0125** |
| | (0.909) | (0.0105) | (0.0136) | (0.00565) | (0.945) | (0.00348) | (0.00634) | (0.00618) |
| Observations | 51,106 | 51,106 | 51,106 | 51,106 | 51,053 | 51,053 | 51,053 | 51,053 |
| $R^2$ | 0.910 | 0.072 | 0.224 | 0.091 | 0.918 | 0.017 | 0.160 | 0.162 |
| | **Results: 4-5 Year Old Firms** | | | | **Results: 6-10 Year Old Firms** | | | |
| **Corp Rate** | 0.807 | -0.000302 | 0.00391 | 0.00432 | -0.0151 | -0.0311** | -0.0199 | 0.0112* |
| | (1.221) | (0.00693) | (0.00876) | (0.00357) | (1.033) | (0.0129) | (0.0162) | (0.00673) |
| **Min Wage** | 0.203* | -0.000478 | 0.000106 | 0.000547 | -0.311*** | -0.000832 | -0.00119** | -0.000388 |
| | (0.117) | (0.000407) | (0.000392) | (0.000434) | (0.0808) | (0.000512) | (0.000532) | (0.000518) |
| **Personal Rate** | -1.575 | -0.00745** | -0.0183** | -0.0109* | 0.738 | -0.00336 | -0.00343 | 0.000211 |
| | (1.309) | (0.00371) | (0.00669) | (0.00598) | (0.796) | (0.00604) | (0.00758) | (0.00871) |
| Observations | 50,975 | 50,975 | 50,975 | 50,975 | 51,198 | 51,198 | 51,198 | 51,198 |
| $R^2$ | 0.913 | 0.018 | 0.147 | 0.148 | 0.955 | 0.017 | 0.156 | 0.169 |
| | **Results: 11+ Year Old Firms** | | | | **Results: All Firms** | | | |
| **Corp Rate** | -1.376** | -0.0470 | -0.0549 | -0.00817 | -1.416** | -0.131 | -0.158 | -0.0278 |
| | (0.539) | (0.0423) | (0.0617) | (0.0292) | (0.461) | (0.0938) | (0.136) | (0.0493) |
| **Min Wage** | -0.0646** | -0.00122 | -0.00237 | -0.00111 | -0.0645** | -0.00490* | -0.00488* | -0.00000342 |
| | (0.0272) | (0.00222) | (0.00199) | (0.00195) | (0.0227) | (0.00284) | (0.00288) | (0.00241) |
| **Personal Rate** | -0.490 | -0.0148 | -0.0730** | -0.0576** | -0.340 | -0.0234 | -0.103** | -0.0801** |
| | (0.671) | (0.0166) | (0.0307) | (0.0246) | (0.428) | (0.0270) | (0.0408) | (0.0281) |
| Observations | 51,258 | 51,258 | 51,258 | 51,258 | 51,258 | 51,258 | 51,258 | 51,258 |
| $R^2$ | 0.994 | 0.013 | 0.130 | 0.115 | 0.996 | 0.025 | 0.214 | 0.191 |

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$
Note: Border Discontinuity results by firm age group.

Table A3: Dropping Counties With Fewer than 1,000 Workers

| | Results: 0-1 Year Old Firms | | | | Results: All Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
| **Corporate Taxes, Minimum Wage, Personal Taxes** | | | | | | | | |
| **Corp Rate** | -2.881** | -0.0337 | -0.0446 | -0.0108 | -1.383** | -0.138** | -0.174* | -0.0367 |
| | (1.199) | (0.0214) | (0.0288) | (0.00865) | (0.443) | (0.0692) | (0.104) | (0.0436) |
| **Min Wage** | -0.0871 | -0.00206** | -0.000865 | 0.00120** | -0.0589** | -0.00357 | -0.00308 | 0.000472 |
| | (0.103) | (0.000651) | (0.000811) | (0.000464) | (0.0241) | (0.00293) | (0.00289) | (0.00243) |
| **Personal Rate** | -0.643 | 0.00182 | 0.00256 | 0.00107 | -0.299 | -0.0276 | -0.0940** | -0.0667** |
| | (0.895) | (0.00929) | (0.0117) | (0.00512) | (0.403) | (0.0234) | (0.0352) | (0.0267) |
| Observations | 61,978 | 61,978 | 61,978 | 61,978 | 62,188 | 62,188 | 62,188 | 62,188 |
| $R^2$ | 0.905 | 0.065 | 0.213 | 0.099 | 0.996 | 0.023 | 0.201 | 0.189 |

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Note: This table reports results using the same border discontinuity method reported in Panel A of Table 4 but drops counties with 1,000 or fewer worker. Results are shown to be quite similar to those in Table 4 which drop counties with fewer than 3,000 workers.

Table A4: Three Years Surrounding Corporate Tax Changes

| | Results: 0-1 Year Old Firms | | | | Results: All Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
| **Corporate Taxes, Minimum Wage, Personal Taxes** | | | | | | | | |
| **Corp Rate** | -4.197** | -0.0359 | -0.0533 | -0.0175 | -1.694*** | -0.129 | -0.134 | -0.00568 |
| | (1.627) | (0.0265) | (0.0341) | (0.0117) | (0.506) | (0.0822) | (0.118) | (0.0505) |
| **Min Wage** | -0.191 | -0.00280** | -0.00192 | 0.000889 | -0.0696** | -0.00140 | -0.00467 | -0.00328 |
| | (0.142) | (0.00124) | (0.00122) | (0.000915) | (0.0290) | (0.00421) | (0.00438) | (0.00298) |
| **Personal Rate** | 0.373 | -0.0380 | -0.0311 | 0.00703 | 1.500** | -0.0939 | -0.132 | -0.0378 |
| | (1.301) | (0.0246) | (0.0280) | (0.0152) | (0.580) | (0.0605) | (0.0908) | (0.0470) |
| Observations | 20,946 | 20,946 | 20,946 | 20,946 | 20,989 | 20,989 | 20,989 | 20,989 |
| $R^2$ | 0.908 | 0.095 | 0.272 | 0.075 | 0.996 | 0.039 | 0.227 | 0.190 |

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

This table reports results using the same border discontinuity method reported in Panel A of Table 4 but focuses on the corporate tax results. For each tax change only the three-year period surrounding the tax change is included. Including the full time period for each state may obscure shorter run effects of the policy.

Table A5: Dropping Distorted Values

| | Results: 0-1 Year Old Firms | | | | Results: All Firms | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) ln(Emp) | (2) Emp Growth | (3) Job Creation | (4) Job Destruction | (5) ln(Emp) | (6) Emp Growth | (7) Job Creation | (8) Job Destruction |
| **Corporate Taxes, Minimum Wage, Personal Taxes** | | | | | | | | |
| **Corp Rate** | -3.142** | -0.0481 | -0.0630 | -0.0150 | -1.415** | -0.130 | -0.158 | -0.0279 |
| | (1.292) | (0.0304) | (0.0412) | (0.0115) | (0.460) | (0.0938) | (0.136) | (0.0493) |
| **Min Wage** | -0.0146 | -0.00168** | -0.000647 | 0.00103** | -0.0691** | -0.00565** | -0.00527* | 0.000360 |
| | (0.0958) | (0.000650) | (0.000871) | (0.000441) | (0.0225) | (0.00280) | (0.00288) | (0.00241) |
| **Personal Rate** | -0.0989 | 0.00860 | 0.00638 | -0.00186 | -0.331 | -0.0229 | -0.103** | -0.0805** |
| | (0.862) | (0.00965) | (0.0126) | (0.00492) | (0.428) | (0.0270) | (0.0406) | (0.0281) |
| Observations | 49,475 | 49,475 | 49,475 | 49,475 | 51,147 | 51,147 | 51,147 | 51,147 |
| $R^2$ | 0.917 | 0.090 | 0.245 | 0.117 | 0.996 | 0.025 | 0.213 | 0.189 |

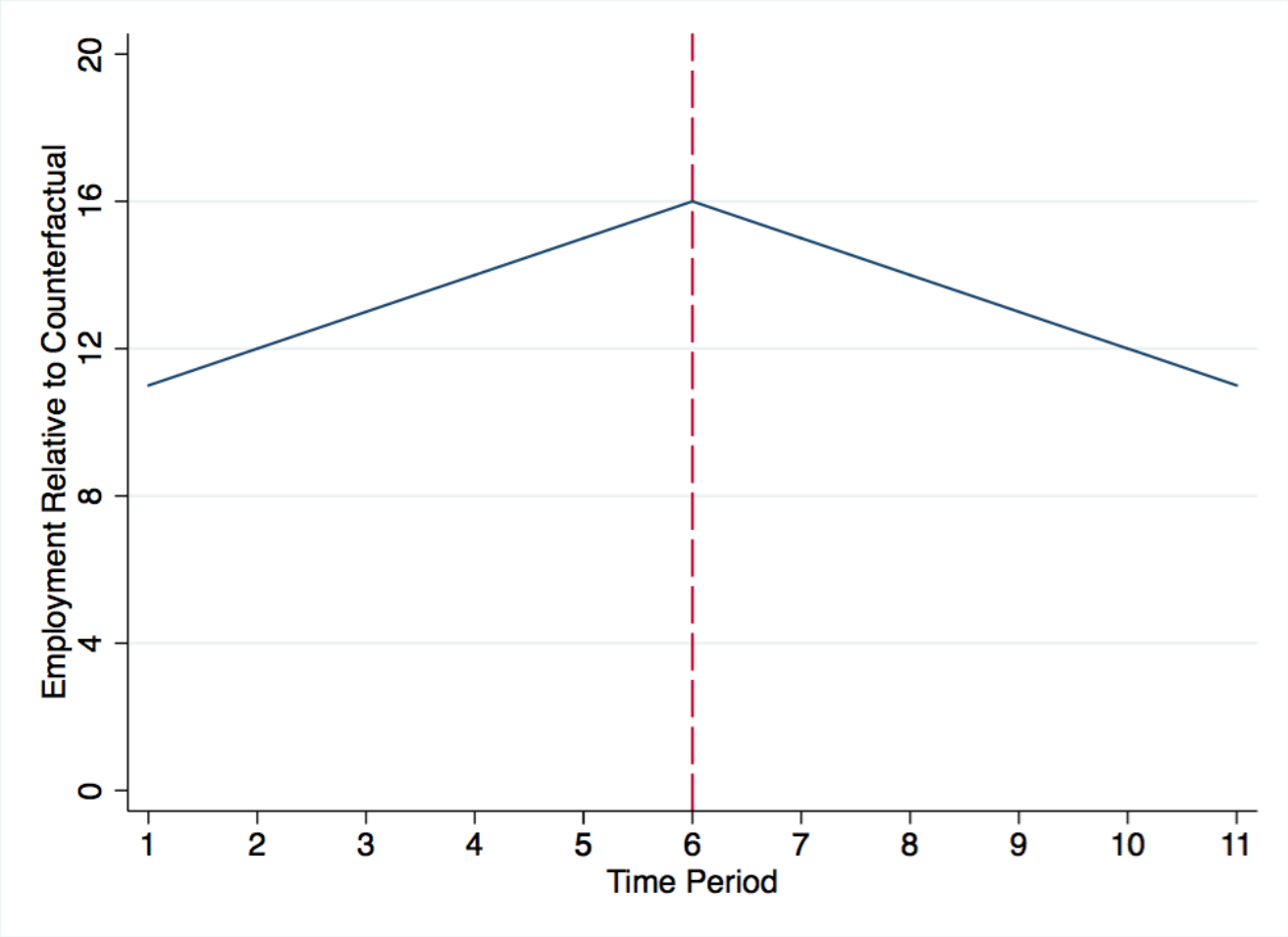
Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$
This table reports results using the same border discontinuity method reported in Panel A of Table 4 but now drops observations that are flagged as distorted in the QWI.

# Table A6: Results by Sector: 0-1 Year Old Firms

| | ln(emp) | | Emp Growth | | Job Creation | | Job Destruction | |
|---|---|---|---|---|---|---|---|---|
| | (1) Beta | (2) SE | (3) Beta | (4) SE | (5) Beta | (6) SE | (7) Beta | (8) SE |
| Construction | | | | | | | | |
| Corp Rate | -1.370 | (2.103) | -0.120 | (0.0676) | -0.173 | (0.108) | -0.0525 | (0.0420) |
| Min Wage | -0.129 | (0.164) | -0.00159 | (0.00309) | -0.00366 | (0.00405) | -0.00230 | (0.00161) |
| Personal Rate | -2.997 | (1.663) | -0.00997 | (0.0664) | -0.0492 | (0.0856) | -0.0409 | (0.0229) |
| *N* | 40362 | | 40362 | | 40362 | | 40362 | |
| Manufacturing | | | | | | | | |
| Corp Rate | 7.898 | (5.764) | 0.0355 | (0.0140) | 0.0419 | (0.0166) | 0.00628 | (0.0144) |
| Min Wage | 0.174 | (0.280) | -0.00622 | (0.00208) | -0.00256 | (0.00160) | 0.00376 | (0.000994) |
| Personal Rate | 4.526 | (4.278) | -0.0821 | (0.0684) | -0.0753 | (0.0361) | 0.0121 | (0.0435) |
| *N* | 19104 | | 19104 | | 19104 | | 19104 | |
| Wholesale Trade | | | | | | | | |
| Corp Rate | 1.740 | (4.307) | -0.0416 | (0.0289) | -0.0409 | (0.0304) | 0.000151 | (0.00763) |
| Min Wage | -0.665 | (0.402) | -0.00427 | (0.00223) | -0.00523 | (0.00214) | -0.00101 | (0.00118) |
| Personal Rate | 12.57 | (4.003) | -0.00745 | (0.0263) | 0.00551 | (0.0258) | 0.0155 | (0.0149) |
| *N* | 17778 | | 17778 | | 17778 | | 17778 | |
| Retail Trade | | | | | | | | |
| Corp Rate | -6.084 | (1.361) | 0.0316 | (0.0195) | 0.0320 | (0.0195) | 0.00184 | (0.00906) |
| Min Wage | 0.131 | (0.235) | 0.0000396 | (0.000957) | 0.000224 | (0.000951) | 0.0000609 | (0.000557) |
| Personal Rate | -0.895 | (2.010) | -0.000583 | (0.0118) | 0.00808 | (0.0124) | 0.00626 | (0.00912) |
| *N* | 46570 | | 46570 | | 46570 | | 46570 | |
| Transportation and Warehousing | | | | | | | | |
| Corp Rate | -1.916 | (3.225) | -0.0773 | (0.0664) | -0.118 | (0.0938) | -0.0427 | (0.0366) |
| Min Wage | -0.0206 | (0.318) | -0.00383 | (0.00388) | -0.00604 | (0.00409) | -0.00222 | (0.00366) |
| Personal Rate | -6.087 | (3.229) | -0.228 | (0.0653) | -0.189 | (0.0824) | 0.0340 | (0.0362) |
| *N* | 22018 | | 22018 | | 22018 | | 22018 | |
| Finance and Insurance | | | | | | | | |
| Corp Rate | -11.49 | (4.006) | -0.0302 | (0.0123) | -0.0220 | (0.0118) | 0.00858 | (0.00769) |
| Min Wage | 0.236 | (0.349) | 0.0119 | (0.00321) | 0.0103 | (0.00296) | -0.00149 | (0.000970) |
| Personal Rate | 4.211 | (4.812) | 0.0391 | (0.0458) | 0.0257 | (0.0429) | -0.0105 | (0.0131) |
| *N* | 13342 | | 13342 | | 13342 | | 13342 | |
| Real Estate and Rental and Leasing | | | | | | | | |
| Corp Rate | -2.760 | (4.262) | -0.0679 | (0.0336) | -0.0571 | (0.0469) | 0.0124 | (0.0297) |
| Min Wage | -0.472 | (0.198) | -0.00385 | (0.00243) | -0.00210 | (0.00278) | 0.00161 | (0.00239) |
| Personal Rate | -3.121 | (3.406) | -0.0250 | (0.0317) | 0.0176 | (0.0374) | 0.0434 | (0.0281) |
| *N* | 15384 | | 15384 | | 15384 | | 15384 | |
| Professional, Scientific, and Technical Services | | | | | | | | |
| Corp Rate | -2.660 | (2.163) | -0.00789 | (0.0279) | 0.00440 | (0.0340) | 0.0130 | (0.0203) |
| Min Wage | 0.0495 | (0.163) | -0.00432 | (0.00505) | -0.00539 | (0.00515) | -0.000761 | (0.00176) |
| Personal Rate | -0.806 | (3.550) | -0.101 | (0.0249) | -0.138 | (0.0332) | -0.0379 | (0.0182) |
| *N* | 27084 | | 27084 | | 27084 | | 27084 | |
| Management of Companies and Enterprises | | | | | | | | |
| Corp Rate | 0.0192 | (2.688) | -0.126 | (0.0881) | 0.0849 | (0.0294) | 0.211 | (0.0716) |
| Min Wage | -0.435 | (0.310) | -0.00296 | (0.00349) | -0.00503 | (0.00313) | -0.00182 | (0.00282) |
| Personal Rate | 9.852 | (3.458) | -0.0934 | (0.0673) | -0.0639 | (0.0577) | 0.0210 | (0.0538) |
| *N* | 21170 | | 21170 | | 21170 | | 21170 | |
| Educational Services | | | | | | | | |
| Corp Rate | 7.268 | (3.705) | 0.00142 | (0.0203) | 0.0141 | (0.0154) | 0.0123 | (0.00955) |
| Min Wage | -0.845 | (0.298) | -0.00470 | (0.00225) | -0.00471 | (0.00216) | 0.0000261 | (0.00123) |
| Personal Rate | 3.490 | (4.423) | 0.00498 | (0.0378) | 0.0171 | (0.0384) | 0.0129 | (0.0138) |
| *N* | 31188 | | 31188 | | 31188 | | 31188 | |
| Health Care and Social Assistance | | | | | | | | |
| Corp Rate | 9.715 | (4.005) | -0.0125 | (0.0609) | 0.0976 | (0.0849) | 0.108 | (0.0420) |
| Min Wage | -0.673 | (0.370) | 0.00147 | (0.00505) | -0.000713 | (0.00745) | -0.000754 | (0.00517) |
| Personal Rate | 3.549 | (4.151) | 0.172 | (0.0555) | 0.202 | (0.0888) | 0.0202 | (0.0463) |
| *N* | 10478 | | 10478 | | 10478 | | 10478 | |
| Arts, Entertainment, and Recreation | | | | | | | | |
| Corp Rate | 1.603 | (2.607) | -0.0769 | (0.0594) | -0.0533 | (0.0614) | 0.0223 | (0.0119) |
| Min Wage | 0.321 | (0.160) | 0.00138 | (0.00249) | -0.000476 | (0.00190) | -0.00174 | (0.00196) |
| Personal Rate | -3.521 | (1.575) | -0.00379 | (0.0292) | -0.0243 | (0.0266) | -0.0232 | (0.0149) |
| *N* | 45010 | | 45010 | | 45010 | | 45010 | |
| Accommodation and Food Services | | | | | | | | |
| Corp Rate | -13.19 | (1.589) | -0.0752 | (0.0206) | -0.167 | (0.0348) | -0.0912 | (0.0200) |
| Min Wage | -0.230 | (0.109) | 0.00138 | (0.00212) | 0.00155 | (0.00276) | 0.000101 | (0.00145) |
| Personal Rate | 1.645 | (2.759) | -0.0287 | (0.0414) | -0.102 | (0.0516) | -0.0654 | (0.0337) |
| *N* | 37418 | | 37418 | | 37418 | | 37418 | |

Note: This table provides results by sector for new firms only. The format of this table differs from past tables in order to display more results. Standard errors are listed next to the coefficients rather than below them. We report results for the largest thirteen sectors in the economy. Importantly, at the county - sector - firm age level, data suppression becomes an issue for the QWI. Smaller sectors have far fewer observations and concerns arise that the policies in question may push observations into and out of suppression. For this reason, we are hesitant to emphasize these results in the paper. However, we believe that they provide suggestive evidence for how policy changes may differentially impact sectors in the economy.

Figure A1: Potential Diff-in-Diff Employment Path




Note: Suppose employment in a particular county takes the above path relative to its counterfactual and that the policy shock occurs in period 6. In this case a diff-in-diff estimate will find no effect of the policy on employment levels because average employment is the same before and after the policy. However, it will find a strong effect on employment growth as there is a clear upward trend relative to the counterfactual before the policy and a clear downward trend after the policy.